Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

Volume XIII. *May, 1904.* *Whole*
Number 3. *Number 75.*

THE

PHILOSOPHICAL REVIEW.

ON MECHANICAL EXPLANATION.

I. *On the Definition of the Mechanical Ideal.* — In philosophy and in science we are frequently called upon to face a certain hypothesis, — the hypothesis, namely, that all the phenomena of the world in which we live are susceptible of a mechanical explanation. In discussing method we are in the habit of referring to this point of view as the 'mechanical ideal.' Now we all feel that in a way we understand what is meant by the mechanical ideal, whether or not we are willing to entertain it, and yet it must be admitted that the literature of philosophy is much richer in instances of an instinctive application of this ideal than in examples of a serious effort to define its meaning. We feel no little confidence in our right to pronounce certain methods of explanation inharmonious with the ideal, but such exclusions still leave us in considerable doubt respecting the inclusion of the term.

For example, it would probably be admitted by all that a biologist who denied the possibility of finding among the physico-chemical conditions of an organism and its environment at any moment the determinants of the growth of the organism at that moment, would definitely have rejected the mechanical ideal. But, on the other hand, would the adoption of a physico-chemical theory of growth be equivalent to the acceptance of the ideal? At least, we can understand the eagerness of an Ostwald to replace the vague concept of "chemical affinity" with a picture whose details are wholly physical of the processes which are involved in neutralization, solution, and so forth. This sympathy may be taken as the expression of an instinctive feeling that the phenomena of chemistry themselves are in need of a mechanical explanation. A like satisfaction attends every successful effort to reproduce certain physical phenomena (for example, those of heat) in terms of concealed mass-motions. In short, a type of explanation which at one stage of our progress and with a view to certain exclusions we may advance as mechanical, will itself appear at another stage to be in need of mechanical explanation. It would thus seem that the use of the concept in question is subject to that vacillation which makes definition of it at once difficult and imperative.

To begin with, the most natural suggestion, and the one most closely in accord with historical development, would view our ideal as arranging the sciences in a series of subsumptions of such nature that we might regard each science as capable of reduction to the one next below it, until at last we arrived at a fundamental science to which all the others might be reduced. The adjective 'mechanical' attached to our ideal would then indicate that this fundamental science was none other than the science of mechanics. Indeed, it would seldom occur to the scientist that there could be any sense in which the phenomena of mechanics themselves were in need of further explanation. If this suggestion be adopted, our task of defining the mechanical ideal will be accomplished when we have given a definition of mechanics and an explanation of the sense in which one science is capable of reduction to another. Such an insight into the meaning of the term having been obtained, we may proceed to examine the grounds which could be urged for the acceptance of the ideal as a guide to our speculation.

In defining the science of mechanics, it is necessary that our method should make use of such differentiæ as are of general application. The problem of the classification of the sciences is very far from having reached solution, but as a contribution to it I may suggest that the characteristics which best distinguish a science are those which, in technical language, are termed the 'dimensions' of the science. The concept of the dimensions of a science, although of familiar application, is not quite easy to define; that is, it is difficult to bring it under the concept of dimensions in general. For our present purpose, it will be sufficient to illustrate the meaning of dimensions and to show in what sense they may be used to differentiate the sciences.

As a particularly simple case, let us consider the dimensions of a system of bodies to which we might give the name of a Laplacean system. Such a system would be defined in terms of the familiar image once offered by Laplace; that is, it would be a system such that, if we knew the masses, the space distribution, and the velocities at all points at any given moment, we should be able to calculate the masses, the space distribution, and the velocities of all points for any other moment. The formula by which such a calculation would be made might be called the axiom of the science dealing with such systems. It is evident that there are four independent observations which must be made at every point in the system, and which must be substituted in the formula, before any determinate problem is presented to us. These four independent observations are mass, length, time, and velocity; and the use to which we put them might suggest an analogy with the way in which we use independent coördinates to determine the position of an element in any dimensional manifold. The concept of the dimensions of our science, however, differs slightly from this, in that we consider not the independent data, but the independent kinds of measurement involved. Thus velocity, being a ratio of length and time, is not regarded as a dimension in the sense now contemplated, but implicitly contains the dimensions length and time; so that in the end the dimensions of a science dealing with Laplacean systems would be mass, length, and time.

With the concept of a dimension now clear, we may proceed to define mechanics as the science whose dimensions are mass, length, and time. We shall, of course, not be understood to identify mechanics with the science of the Laplacean systems in the sense of the preceding illustration, for, while such a science would certainly be mechanical, the converse is not implied, that mechanics is the science of Laplacean systems. If, in fact, we were to compare this definition with the contents of an ordinary textbook of mechanics, we should see that our definition was both broader and narrower than that which is implied in the subjects there treated of. It is broader for the reason that it would include such widely divergent systems of mechanics as those based on the theory of rigid connections, on the one hand, and those based on the theory of action at a distance, on the other. It is narrower in that it would exclude certain problems which are generally handled in text-books on mechanics and yet which we cannot regard as properly mechanical, — for example, the problems of impact. For evidently no knowledge of the masses, space distribution, and length-and-time quotients, would inform us whether two colliding bodies would behave as elastic or as inelastic bodies. Without this knowledge, however, the problem of impact is indeterminate. The knowledge itself can only be conveyed in terms of a coefficient of elasticity, which must at present be regarded as a new dimension. The breadth, however, is evidently proper to a definition which is to include the common feature of all schools of mechanics, without taking sides on questions of detail. The narrowness succeeds in relegating to the domain of general physics phenomena that are generally recognized as lacking a purely mechanical solution.

Mechanics, then, is the science whose dimensions are mass, length, and time; it remains to be seen what is meant, when we speak of reducing other sciences to mechanics. Our method of offering such an explanation must depend upon the acceptance of our suggestion that the various sciences may be differentiated in terms of their dimensions. This suggestion requires some defense. It will be seen at once that it is neatly applicable to the definition of certain recognized branches of physics. For example, thermodynamics would involve the additional dimension *temperature;* electrostatics and electrodynamics, the additional dimension *quantity of electricity;* magnetism, the dimension *strength of pole.* But it will not at once be evident that there is any sense in which we could define chemistry in terms of a specific dimension or group of dimensions, and a like difficulty would pertain to the definition of biology, psychology, sociology, etc.

As for chemistry, we must distinguish between its condition in the past, in which it presented a series of more or less general observations which could not be united in any single formula, and a tendency towards systematization which characterizes its present. There are, it would seem, two main problems of chemistry: (1) to deduce the properties of a compound from the properties of the elements entering into it; (2) to develop a formula by which the various properties of elements may be expressed as functions of one of their number, which may then be taken as defining the element.

In connection with the first of these problems, Ostwald has divided the properties of compounds into the 'additive,' the 'constitutive,' and the 'colligative.' The 'additive' properties of a compound are the simple sum of the properties of the elements combined: thus the molecular mass is the sum of the atomic masses. The 'constitutive' properties are those which depend not only on the elements combined, but upon a factor which is usually called the 'arrangement' of these elements. The 'colligative,' finally, depend wholly upon the arrangement of the elements. If, now, as Ostwald suspects, it should be found that the 'constitutive' and 'colligative' properties are ultimately reducible to the 'additive,' or if the factor which is termed 'arrangement' may be conceived to depend on the space distribution,— or space order, let us say,— the whole problem of the properties of compounds presents no dimension which does not belong to the elements themselves. If, on the other hand, the reduction of 'constitutive' and 'colligative' properties to 'additive' cannot be effected, or if the factor of 'arrangement' cannot be conceived in spatial terms, it is possible that the science of chemical compounds would possess a specific dimension of its own.

Again, the immediate result of the attempt to express all the properties of elements in terms of one of their number taken to be characteristic, is illustrated in the formulation of the periodic law. Imperfect as this scheme is recognized to be, it was still possible for Mendelyeff to predict the properties of an element as yet unobserved from the assumption of its atomic mass, and to find his prediction confirmed by later observation. The possession of such a formula as the periodic law suggests would have a tendency to make atomic mass the dimension of chemistry.

May we not, therefore, say that in so far as chemistry succeeds in being a single science rather than tables of collated observations, that single science is definable by a specific dimension ? And conversely, in so far as we are unable to assign any dimension to chemistry, does not the application of a single name to entirely independent observations depend rather upon an association of ideas, upon accidental similarity of method, than upon any right to regard that name as capable of a unique definition ?

As to the other sciences mentioned, biology, psychology, sociology, etc., it is clear that they are interested in laws which apply to complex wholes. The terms in which these laws are stated are in general not applicable to the parts of which the wholes are composed. The question, then, as to whether these sciences are definable in terms of specific dimensions, is not identical with the question as to whether they are definable at all. If, for example, it were admitted that the phenomena of organic life could not be explained in terms of the physical and chemical constituents of an organism, it might be possible that a study of biology would lead to the discovery of a dimension which the physics and chemistry alluded to had not included. So, for example, it has been suggested that 'vital force' might be regarded as a property, related, say, to magnetic force as magnetic force is related to gravitation. To appeal to such a force would be to attempt to give biology a specific dimension. But if no such appeal is made, and if the biologist admits that the laws of the totals with which he is dealing can be constructed out of the physico-chemical laws of the parts which compose them, the science does not in the least cease to be definable, but it ceases to be an independent science. Its definition must now be sought in the nature of the totals or groups with which it deals. For this reason I am in the habit of referring to a science thus defined as a 'superimposed, science. It will be seen, then, that the differentiation of the sciences in terms of their dimensions is a differentiation which is only meaningful in case these sciences are independent ; and conversely, to define a science as a 'superimposed' science is to admit that it possesses no specific dimensions. For the rest, we are not interested in the question as to whether biology, psychology, and sociology are really 'superimposed' sciences or not.

Enough has perhaps been said to make it clear in what sense our suggestion that sciences may be differentiated in terms of their dimensions is applicable throughout the whole range of independent sciences. The advantage of this method of differentiation is that it yields us immediately the statement for which we have been in search, of the meaning of reduction. We may now say in general that *any science* x, *dimensions* abcd, *is reducible to any science* y, *dimensions* abc, *when it may be shown in any manner that the term* d *is expressible as a function of* abc. For example, let x be the science of thermodynamics, whose dimensions are mass, length, time, and temperature, and let y be the science of mechanics. The reduction of thermodynamics to mechanics is effected when we show that temperature is a function of mass, length, and time, or of any pair of these three terms. This reduction is exactly the one that has been effected by the mechanical theory of heat, in which it has been made to appear that temperature is a function of the velocity of certain concealed mass-motions. It would be easy to find in the physical speculations of our day other reductions of an exactly similar nature. Such a reduction having been made, the reduced science loses its independence with its specific dimension, and if retained in our thinking at all, must be treated as 'superimposed' science.

Thus we obtain, as the most general statement of the mechanical ideal, the hypothesis that *mass, length, and time, are the dimensions of natural science.*

II. *On the Possibility of the Mechanical Ideal.* — Having defined the mechanical ideal in a way that has at least the advantage of displaying its own motives, we are now in a position to consider the arguments that may be advanced for its acceptance or its rejection. We may at once lay aside as irrelevant all reference to our present accomplishment in the premises. It is obvious that we are indefinitely remote from the realization of the ideal as it has been defined; it is no less plain that many steps of modern progress might readily be looked upon as conducting us toward such a goal. But if no discussion of this problem save the *a posteriori* is possible, our only business for the present is to possess our souls in patience and to await the results of experimental science. As an historical fact, however, there have been advanced reasons purporting to be *a priori* for supposing that the attainment of our end is impossible, and other reasons laying no less claim to an *a priori* character for expecting with confidence its ultimate realization. It is to a consideration of these *a priori* grounds for acceptance or rejection that we now turn. In the present paper I shall confine myself to the former class of arguments, reserving the discussion of the latter class for a future occasion.

That a mechanical image of nature can never be constructed has been urged on one of two grounds, — either on the ground that the image is self-contradictory and so meaningless, or on the ground that it is essentially untrue to nature. The former objection goes back to Parmenides and Zeno ; it has never lacked representatives. The latter has been insisted upon most obstinately by those who have been impressed with the multitude of purposeful processes in nature, and who cannot convince themselves that nature could be described or its happenings predicted without making use of expressions that have reference to ends ; but such reference, they feel, implies other laws than those which enable us to define a mechanical system.

Such objections to the meaningfulness of the mechanical image as turn on the difficulties in defining mass, length, time, and their combinations, cannot be discussed in this connection ; we should find ourselves involved in some of the most perplexing chapters of metaphysics. Yet we are not prevented from taking at once a certain attitude toward this class of objections. To any one who thinks that he has discovered contradictions or insufficiencies in the definitions ordinarily offered in the field of geometry, kinematics, and mechanics, we can only reply that it would be surprising if such imperfections were not to be found. The history of the search for definitions from Socrates to the present time makes nothing plainer than that the terms we use most instinctively are the ones whose meaning it is most difficult to set forth. But on the whole we make progress. The particular inadequacy of mass, as defined by Newton, is not to be found in Mach's definition. Hertz, while admitting this, is still dissatisfied with the accomplishment of Mach, and if Hertz is not justified, nothing is more likely than that another critic will be. There is every reason to hope, however, that since the modern systematist has detected and removed the imperfections of Newton, the future critic will be able to detect and to remove the flaws that are latent in our current system. The history of the concept of mass is repeated in that of the other dimensions. No one who is acquainted with the problem of framing the axioms of geometry and kinematics is going to stake much on the perfection of any system that has yet been advanced, but neither can one find any single difficulty which from Euclid to Hilbert and Poincaré, for example, has not been overcome.

We may then take this attitude toward the first class of *a priori* objections to the mechanical ideal, namely: that if no definition of the terms in which we have presented this ideal is beyond danger of attack, yet no one inadequacy has been discovered which has remained beyond remedy.

Now let us turn to the second class of *a priori* objections. They are advanced by the heirs to the Aristotelian doctrine that "everything in nature takes place for the sake of an end." It is not easy to determine just how broad and just how narrow was the 'nature' contemplated by Aristotle, nor yet to what extent things taking place for the sake of an end were also, in his view, parts of a mechanism. But in the sequel the possession of a nature that could be defined in the terms of the end sought and "always or for the most part" attained, was frequently enough supposed to demonstrate the inadequacy of mechanical explanation. Thus Aquinas: "We see that certain things lacking perception, sci. natural bodies, act for the sake of an end . . . But things which have no perception can only tend toward an end if directed by a conscious and intelligent being. Therefore there is an intelligence, by which all natural things are ordered to an end."[1]

---

[1] *Summa theol.*, I, quæst. 2, art. 3.

The most significant modern representative of the point of view which Aristotle sought to make final is the science of biology. It was in this field, it will be remembered, that Kant imagined the demonstration of the inadequacy of mechanism to be complete. "It is quite certain," he writes, "that we can never adequately know, still less explain, organisms and their intrinsic possibility in terms of the purely mechanical principles of nature. It is so certain, indeed, that it is an absurdity for men even to make the attempt, or to hope that another Newton may arise who could make so much as the production of a blade of grass intelligible in terms of natural laws that are not directed by a purpose. An insight of this kind must be absolutely denied us."

Perhaps the most helpful way of studying the present attitude of biology toward this question is to sketch its recent history, or at least a typical phase of that history. There is nothing more characteristic of the mechanical ideal in its practical working out than the effort to divide the larger bodies with which our experience presents us into spatial parts, to accord to these parts as few attributes as possible, then to seek to reconstruct the original body out of these *primordia rerum.* In biology the structure that first suggested itself as a convenient unit of composition was the cell, and the method which considered the cell to be related to the organism as the Democritian atom is related to the body composed of such atoms has been called the 'cell theory.' The distinct formulation of the cell theory goes back to Schleiden and Schwann. In 1838 Schleiden, confining his attention to plants, writes: "Each cell leads a double life, an independent one pertaining to its own development alone, and another incidental in so far as it has become an integral part of the plant." In 1839 Schwann extends the concept to all organisms: "Each cell is within certain limits an individual and independent whole. The vital phenomena of one are repeated entirely or in part in all the rest. These individuals, however, are not arranged side by side as an aggregate, but so operate together in a manner unknown to us as to produce an harmonious whole."[1] And again, "The whole organism subsists only by means of the reciprocal action of the single elementary parts."

---

[1] Taken from Whitman, "Inadequacy of the Cell Theory of Development," *Journal of Morphology*, Vol. viii, pp. 639 ff.

Except for an occasional vagueness, such as the reference to "an harmonious whole," and except for the substitution of the 'life' of a cell for the mere 'existence' of an atom, the preceding description might have served Newton to depict the anatomy and physiology of all physical bodies, merely changing 'cell' into 'atom.'

Of course, the cell theory is not yet mechanical, since it merely assumes the living cell, and in connection with it implies terms of description and explanation that are not immediately susceptible of mechanical definition, nor even of physico-chemical definition. Yet since the phenomena of cell life are to a much greater extent capable of a physico-chemical treatment than those of the organism as a whole, it is natural that the cell theory should be looked upon by those who defend it, as well as by those who oppose it, as an effort in the direction of mechanical explanation, and that it should seem to an onlooker that a biology which found itself to be drifting away from the cell theory had abandoned the hope of mechanical explanation in its field. That biology is taking this course is the view of some of its most prominent representatives.

The writers in question, differing as they do on points of detail, are at least agreed on this proposition: That we know no laws of the individual cell or of the interaction of cells such as would explain the behavior of that aggregate of cells we call an organism. Some, at least, of the laws of the organism must treat it as indivisible. A favorite figure of those who take this standpoint,—the "organism standpoint," as Whitman calls it,—is borrowed from chemistry. "It can be shown, I think," says Morgan, "with some probability, that the forming organism is of such a kind that we can better understand its action when we consider it as a whole and not simply as the sum of a vast number of smaller elements. To draw . . . a rough parallel; just as the properties of sugar are peculiar to the molecule and cannot be accounted for as the sum total of the properties of the atoms of carbon, hydrogen, and oxygen of which the molecule is made up, so the properties of the organism are connected with its whole organization and are not simply those of its individual cells, or lower units."[1] So Whitman compares the organism of many cells to a complex molecule. "The complex unit bears not only the structure of its individual parts, but also a totally new structure formed by the union of these parts."[2]

The concrete facts which these statements are intended to summarize are these:

1. The relation between two structures which the biologist calls 'homology,' may exist between a unicellular and a multicellular body. "So far as homology is concerned, the existence of cells may be ignored."[3]

2. In the process of development a unicellular organ may replace in one organism a multicellular organ in another. The laws of growth of an organism must be formulated in terms of the organism as a whole, and not in terms of its cells, if we are to have "continuity of organization." "Continuity of organization means only that a definite structure foundation must be taken as the starting-point of each organism, and that the organism is not multiplied by cell division but rather continued as an individuality through all the stages of transformation and subdivision in the cells."[4]

3. The important phenomena of regeneration.

(*a*) The phenomena of 'polarity.' "We find that a piece of a bilateral animal regenerates a new anterior end from the part that lay nearer to the anterior end of the original animal, a new right side from the part that was nearest the original right side, and a new dorsal part from the region that lay near the original dorsal part, etc." Since the character of the cells constituting the two surfaces of a single section cannot greatly differ, the nature of the growth on them must be due to the "structural relation of each to the whole to which each belongs."[5]

(*b*) The phenomena of growth. For example, in the growth of the tail of a fish after an oblique section, that part is found to grow the faster which has the greater growth to accomplish before it recovers its normal proportion to the other dimensions of the original. "These results show very clearly that in some way the development of the typical form of the tail influences the rate of growth at different points. Although the physiological conditions would seem to admit the maximum rate of growth over the entire cut-edge, this only takes place in those parts that give the new tail its characteristic form."[1]

---

[1] *Regeneration*, p. 278.

[2] Whitman, *loc. cit.*, p. 641.

[3] *Ibid.*, p. 645.

[4] *Ibid.*, p. 646.

[5] Morgan, *loc. cit.*, p. 280.

So much for the organism standpoint and the concrete facts upon which it is based. Whether it does or does not present an obstacle to the realization of a mechanical ideal, depends upon the way in which it is interpreted, and, so far as I can discover, three constructions have been put upon it.

1. The cell being unsuitable to serve as a biological element which, itself without organization, produces an organism by division, combination, and interaction, a smaller unit is sought. "If the formative processes cannot be referred to cell division, to what can they be referred? . . . The answer to our question . . . will find the secret of organization, growth, and development not in cell formation, but in those ultimate elements of living matter for which *idiosomes* seems to me an appropriate name. What these idiosomes are . . . is the problem."[2] Such an outcome means that the organization standpoint is far from being a step away from the mechanical ideal; instead of posing the problem of physico-chemical explanation when analysis has been carried as far back as the cell, the whole discussion is postponed until we have arrived at the 'idiosome.' So understood, the organization standpoint means to correct, not the ideal of a biological unit, but the identification of the cell with that unit.

2. A second point of view is that defined latterly by Driesch, to whom the phenomena which we have referred to as organic appeal with particular force. The laws of regeneration and growth are not to be found in the properties of the cell, nor of any smaller organic element, nor of the inorganic constituents of

---

[1] *Op. cit.*, p. 133.

[2] Whitman, *loc. cit.*, p. 657.

organic matter. They "do not fall within any type of law known to the inorganic sciences, but require us to assume a new, peculiar, and peculiarly evidenced kind of elementary (that is, not further analyzable) law, and this necessity results from the fact that no physico-chemical mechanism can be imagined by means of which the phenomena in question can be reproduced."[1] The observations upon which Driesch bases so important a conclusion are not particularly recondite. Organisms can be found which have the following properties: (1) from any part the whole may be regenerated; (2) any part can be made to yield any part of the regenerated whole.[2] These characteristics give rise to two reflections. In the first place, the phenomenon of regeneration here studied cannot be subsumed under physico-chemical laws. For, observe the regeneration of any segment; at some point of the segment differentiation begins. If we are to explain the process in physico-chemical terms, either this point must differ in physico-chemical structure from its neighbors, or it must be differently stimulated from without. But the latter alternative is easily excluded by experimental control. Nor can the former be true, since any neighboring point could have been made the seat of differentiation by properly choosing the site of section. In the second place, the laws which the process of regeneration actually does obey are not mechanical, as may be seen from the following analysis of them.

Suppose we were given the problem to predict the point at which differentiation would occur in a given case. What data should we need, and what type of formula should we make use of? We should have to know (1) the type of organism to which the experimental fragment belonged, (2) the stage of the growth of each part operated upon, and (3) the site of the operation. We may, I take it, conceive the first datum to be given as a system of ratios, each point in the organism being characterized by the ratios of its distance from certain determinate points (say the poles of the axis or axes of symmetry). Such ratios are

---

[1] Driesch, "Die Lokalisation morphogenetischer Vorgänge," *Archiv f. Entwickelungsmechanik der Organismen*, Vol. viii, p. 99.

[2] To both of these statements there are obvious limits, which, however, do not affect the present discussion. Cf. *loc. cit.*, p. 72 f.

obviously independent of the absolute size of the mature organism, or of any other peculiarity of the individual case experimented upon. The second datum is given in the same way, though, unlike the first, it depends on time as a variable, or at least upon the typical ratio of the time taken to acquire a given form to the time taken to attain the typical form represented in (1). For any given experimental case data (1) and (2) are evidently of the nature of fixed parameters, and the point of differentiation will depend on (3), the site of the operation, as the only variable. We may readily imagine the working out of the formula in an illustrative case. Suppose the segment resulting from the experimental operation were a tube, and that the first differentiation "necessary to pass from this form to the type-form (1)" were recognized to be a constriction of the tube, we may imagine that our formula would yield us a coefficient dependent upon (1) and (2) and an absolute dimension, say the length of the single axis of symmetry from section to section determined by (3). We should then locate the constriction at a distance from one pole of the axis equal to a fractional part of the whole length of the axis, the value of this fraction being the coefficient calculated from the formula.

A science which makes use of such formulæ as the foregoing must be, in Driesch's opinion, *sui generis.*

There are many points in Driesch's article that would make interesting topics for discussion, *e. g.*, his conception of the 'type' as the 'end' of regeneration and growth, to attain which a given differentiation is 'necessary'; but the whole concept of end and of necessary means is better left for another occasion when it may be given fuller treatment. For the present, we may content ourselves with examining the two main theses of the argument as now explained. The first maintained that whatever the laws determining differentiation might be, they could not be physico-chemical; the second supposed itself in possession of these laws, and pointed out that they were not physico-chemical.

As to the first, let it be admitted that there is no difference definable in physico-chemical terms between the point at which differentiation takes place and its fellows. There is yet a difference definable in geometrical terms, and with this must come a difference in the kind of stimuli affecting the point.[1] A somewhat analogous case is presented in the phenomenon of magnetization. Any point of a soft iron core may be made a pole by properly sectioning the piece, yet the piece of iron is physico-chemically homogeneous and we select a constant magnetic field. It is exactly its geometrical peculiarity that differentiates the physical conditions at this point from those that exist at neighboring points.

The second consideration points to the laws that determine differentiation, and shows that they are not physico-chemical in their nature. The chief distinction is that these laws state the processes that take place at one point to be a function of its geometrical relation to other remote points, making no mention of the structures that are located between these points.[2] In the fact that the laws of biology do neglect certain details, I think that Driesch has put his finger on that which characterizes biology as a science, and the peculiar way in which this elimination is effected ought to serve as a definition of this science. But the fact that by a process of elimination we can obtain laws in which new kinds of data are demanded, new kinds of formulæ used, does not mean that we have a new science, or, in the terminology of this article, does not show that we have introduced a new dimension. Nothing is more common in the handling of purely mechanical problems than to effect just this kind of elimination. Thus, to take one case, by calling approximately rigid connections absolutely rigid, we are able in mechanical systems to 'eliminate' coördinates, that is, to neglect detail. As a result of this elimination, we frequently obtain formulæ which introduce new terms. The law, "the work we can get out of a machine is equal to the work we put into it," is such a formula, and can be applied in practice to the measure of internal work without considering at all the construction of the machine.

---

[1] We here accept Driesch's contention that the stimulation of a point by its neighbors is as much to be accounted stimulus (as opposed to structure) as is the stimulation from causes quite independent of the organism.

[2] This I take to be the chief outcome of Driesch's demonstration of vitalism. His use of the concept 'action at a distance' is a help to the imagination to which the author is entitled if he be not confused thereby, and I see no evidence that Driesch has attached any undue importance to the device.

In a word, Driesch does not show physico-chemical explanation to be impossible in the field of biology, and does not convince us that the formulæ here used are other than such as would result from eliminating detail in the physico-chemical process, after a fashion that is perfectly familiar to us.

3. The foregoing criticism of Driesch may be taken as a fitting introduction to the third interpretation of the organism view,—the one which, so far as the present writer has observed, is the most common among the biologists of the day. This view admits the existence of laws peculiar to biology, making it for the present an independent science in the sense that no knowledge of the physics and chemistry of the cell or of any other unit will enable us to replace these laws in the business of prediction. But though these laws may at present be indispensable and irreducible, though they may be permanently true and useful, the establishment of their existence cannot constitute a 'demonstration' of the vitalistic standpoint in the sense urged by Driesch. In spite of the absence of a physico-chemical explanation of such phenomena, is there any reason to suppose such an explanation to be impossible? Morgan sums up the data upon which we can base an answer, as follows. (1) The action of poisons, the formation of galls, the effect of lithium salts (Herbst), changes due to light, gravity, contact, etc., are best understood from the physico-chemical standpoint. (2) The effect of 'internal' factors is less easily brought under this point of view. Thus the growth of an egg "we find difficult, if not impossible, to attribute to external causes, yet . . . the first steps through which this takes place can be referred to physico-causal principles. These are the separation of the piece from the whole; the change of the unsymmetrical piece into a symmetrical one, brought about, in part at least, by contractile phenomena in the piece, aided, no doubt, in some cases by surface tension, etc. . . . We find here the beginning of a physico-causal change, and . . . we have no reason to suppose that at one step in the process this passes into the vitalistic causal principle." Having insisted upon the present

impossibility of offering a complete physico-chemical explanation, the author concludes: "Shall we, therefore, call ourselves vitalists? . . . I see no ground for accepting a vitalistic principle that is not a physico-causal one, but perhaps a different one from any known at present to the physicist or chemist."[1]

The preceding sketch of a certain phase of development in biological science has been given in the belief that it is here, if anywhere in experience, that we must look for facts that promise ultimately to resist mechanical explanation. If such facts were unanimously urged by the leading biologists of the day, one would still accept their conclusion with caution, realizing how difficult it would be to form an opinion as to what is 'ultimately' possible and impossible. But as it is, the weight of technical opinion, and of that branch of technical opinion which is most impressed with the error of certain hasty steps leading too directly toward the *quasi*-mechanical theory of life processes,— the weight of this opinion will recognize neither a 'demonstration' nor a balance of probability in favor of the failure of the mechanical ideal. If a layman may venture to estimate the best biological opinion, it would sum up to this: Laws which are not mechanical, such as those having reference to ends (Pflüger and Wolff), and those employing concepts like *actio in distans* (Driesch), are valuable in biology and make prediction possible where it would not be possible if we were to confine ourselves to mechanical terms; but this value is either temporary, while we await a better mechanical insight (Haacke), or if permanent, it is in the nature of an economic device. In any case, the existence of non-mechanical laws does not excuse us from the search for more elementary mechanical laws; still less does it give an assurance that such a search must remain permanently unsuccessful.

The writer has advanced the opinion that if the inadequacy of the mechanical ideal cannot be demonstrated from those aspects of nature studied by the biologist, then in no other region of experience can we expect to find such a demonstration. This opinion must be left for the present as a conjecture based on experience; the present paper does not pretend to have exhausted all the historical motives that have led thinkers to oppose the mechanical ideal. For example, it takes no account of the large body of opinion which opposes to the mechanical, not another kind of law (teleological, vitalistic), but the alternative of no law at all. In some of its aspects the doctrine of *liber arbitrium* would have to be so interpreted.[1] But these more general problems would carry us beyond the regions we could profitably discuss in brief space. We must, then, be content with the best example of opposition to a mechanical ideal with which history presents us, and pass on to a new question. If, namely, we can find in experience no obstacle to our progress in the direction indicated by the mechanical ideal, can we find any reason for supposing this progress to be necessarily continuous? Or, again, if we were to attain the goal defined, should we have reached the final solution of the problem of explanation? In a word, if there is no justification in present knowledge for predicting the failure of the mechanical ideal, is there any safer ground for predicting its success? But this chapter of the discussion must, as has been said, be reserved for a future occasion.

---

[1] *Loc. cit.*, pp. 285 ff.

EDGAR A. SINGER, JR.

UNIVERSITY OF PENNSYLVANIA.

---

[1] Perhaps the standpoint taken by Renouvier and Piat in their *Nouvelle monadologie* may be taken as giving the most systematic presentation. On this see the author, PHILOSOPHICAL REVIEW, Vol. viii, pp. 638 f.